



## Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact [support@jstor.org](mailto:support@jstor.org).

## DISCUSSION AND CORRESPONDENCE

ON "SOMA INFLUENCE" IN OVARIAN  
TRANSPLANTATION

TO THE EDITOR OF SCIENCE: May I take space in your columns for a brief discussion of the matter which Professor Guthrie presents in your issue of May 26, the diametrically opposite conclusions as regards the effects of ovarian transplantation reached, on the one hand by himself and, on the other, by Dr. Phillips and myself?

Beyond the point of clearly stating the essential difference in our conclusions and the ground on which this difference rests, I take it, neither Professor Guthrie nor I would care to go in the way of discussion.

The question at issue is first of all one of the sufficiency or insufficiency of evidence. Guthrie says regarding his own work (p. 816): "The primary object of the experiments was to determine if an engrafted ovary might retain its reproductive function. . . . And incidentally information on soma influence was secured." The incidental result happens to be the one of more general interest, but is impossible without the survival and functioning of "an engrafted ovary." So that the whole discussion narrows itself down to this: Has Guthrie presented adequate evidence that in his experiments an engrafted ovary *did* survive and function?

The facts are these. He transplanted the ovary of one hen into another hen. The second hen afterward laid eggs. Does it follow that the eggs came from that transplanted ovary? Not unless it can be shown that there was no other possible source from which they could have come.

What should we say to this sort of evidence? A boy rushes into the house. "Father," he says, "I have killed a hen." "How do you know, my son?" "Why, I threw a stone over the fence into the henyard, and when I opened the gate and went in, there lay a dead hen." Is that proof that the hen was killed by the stone which the boy threw over the fence?

To prove Guthrie's conclusion two facts

must be established neither of which has he made any attempt to establish. These are, first, that the introduced tissue survived; and second, that no other ovarian tissue was present in the engrafted animal. Our own experiments show that in guinea-pigs engrafted ovarian tissue *taken from another animal* survives in only a small percentage of cases, and further that complete castration of the female is difficult. Even though every apparent vestige of the ovary is removed, nevertheless a functional ovary may later be developed at the original ovarian site. This possibility for fowls Guthrie ignores, yet in fowls complete castration would seem to be a much more difficult matter than in guinea-pigs, because of the diffuse nature of the ovary and its close adherence to large blood vessels. It seems to me essential to Guthrie's contention that he should establish his ability completely to castrate a hen. This he has not done. For if the hen can not be castrated, what warrant have we to speak of somatic influence in the offspring of grafted hens, these offspring being of doubtful origin? I can think of only two ways in which the survival of engrafted ovarian tissue could be established, viz., (1) by transplanting the ovary into some situation other than the normal one and subsequently demonstrating its existence there by autopsy and histological examination. This, the most direct and certain method, we have used with success in a number of cases, as have also several earlier investigators, whose work has been reviewed by Dr. Phillips and myself. Guthrie is prevented from employing this criterion by his uniform practise of transplanting the ovary to the original ovarian site. There is left to him only the criterion next to be mentioned, viz., to judge by the character of the young produced by the grafted animal. He finds in general that the young strongly resemble the grafted mother. Now, this fact admits of two interpretations, one of which Guthrie offers; the other has been offered by Phillips and myself. Guthrie holds that the introduced ovarian cells changed their character to conform with that

of the animal in which those cells were contained; we hold that it is unnecessary to assume such a change, but that the young were like the mother because she *was* their mother, and that they developed from *her own ova*, not from those introduced. We have shown elsewhere by a detailed examination of the facts reported by Guthrie that there is nothing in them at variance with the known facts of color inheritance in fowls, if it be supposed that in these experiments the mother furnished her *own ova* to produce offspring. But if it be supposed, as Guthrie does, that the ova came from an engrafted ovary, then serious contradictions are encountered as regards the color inheritance. Such contradictions Guthrie may not lightly push aside by disclaiming any interest in laws of inheritance on the ground that they are of "no concern" to him. He who claims to have modified inheritance should know what *normal* inheritance is, and he can not divert attention from chickens by scornful references to "peas," nor from stubborn facts by thrusts at "theories built largely upon speculation." No theories are involved in this discussion except the one which Guthrie has propounded, that inheritance is affected by foster-mother influence. We are concerned merely with facts which may either substantiate or disprove this hypothesis. It happens that the subject of color inheritance in fowls has been an object of careful study by several competent observers for a number of years, and we have a large body of data on the normal inheritance of black and white in crosses of fowls. Is it wise in discussing a supposed case of modified color inheritance in fowls to disregard this data as of "no concern"? Is breeder's evidence of "no concern" in a question of modified breeding?

To sum up in a few words our criticism of Guthrie's "evidence of soma influence," we hold that no satisfactory evidence of such influence has been produced because first, it has not been shown that a hen can be completely castrated, but if this can not be done, we can not be certain that eggs discharged from the ovary were really derived from intro-

duced tissue and not from a regenerated ovary. Secondly, it has not been shown that in Guthrie's experiments the transplanted tissue actually persisted. Without the fulfillment of *both* these conditions no transplantation experiment can be considered critical.

Guthrie calls attention to the fact that in an early announcement of his results he drew only provisional conclusions. This is quite true; they were in their entirety as follows:<sup>1</sup>

1. "The ovaries transplanted in these chickens seemed to function in a normal manner."

2. "The color characters of the resulting offspring appeared to be influenced by the foster-mother."

No exception can be taken to these modest conclusions. No claim is made in them of more than a *seeming* persistence of engrafted tissue and an *apparent* modification of the color characters of the offspring, which however at the present time we are in a better position to explain.

If we are to understand that in the present paper Guthrie means merely to reassert these original conclusions, I make no objection to them.

It did *seem*, as Guthrie stated, that in his experiments the transplanted ovaries functioned, but that is no proof that they *did*. Our criticism of Guthrie's results is directed merely toward establishing this point. Doubtless it *seemed* to the boy who threw the stone into the poultry yard that he had killed the hen, but I doubt whether his father would have accepted that conclusion without some independent investigation. Such an investigation of Guthrie's results, Phillips and I have made.

In a case which we have fully described elsewhere the two criteria of the persistence and functioning of transplanted ovarian tissue which have been enumerated are, I believe, adequately met. That Guthrie does not share this view is of little consequence in this connection, but in stating his reasons for dissent Guthrie, doubtless inadvertently,

<sup>1</sup> *Journ. Exp. Zool.*, 5, p. 571.

misstates certain facts which I can not pass over in silence, lest this be interpreted as assent. He states, first, that we used "mongrel stock." "Therefore, any evidence furnished by the character of the offspring would be of doubtful value." On what Guthrie bases this statement I am unable to discover. It is wholly contrary to fact. We described in the body of our paper "one successful case" and in a postscript a second case complete except as regards the autopsy. In describing the successful case, p. 8, the statement is expressly made that the ovary was taken from "a pure black guinea-pig." This guinea-pig belonged to a family of coal-black animals which I have had for about seven years. This family is descended without admixture of other blood from three original individuals, a male and two females, all intensely black, the progeny of which have been closely inbred now for several generations without ever producing any observable deviation from the solid black type of the progenitors. The albino grafted was also of pure race, one which I have bred for about ten years. The albino male with which the grafted animal was mated was of a different strain, but of known and tested gametic composition, so that I can state with much positiveness the kind of young which he produced (and would regularly produce) in matings with guinea-pigs of different color varieties.

The second successful case, described in a postscript, concerned a color variety which I originated, the brown-eyed cream, and which breeds very true, since all the color factors which it contains save one are recessive in nature. This variety *can* produce only one variety of colored young. It is the *ultimate recessive* colored variety of guinea-pig. Having originated this variety some years ago and bred it pure and in crosses ever since, I think I may justly claim to know something about its behavior in inheritance. In neither of the cases which we have described as "successful" was an animal used whose breeding capacity was not definitely and fully known, as definitely as we know what will happen when oxygen and hydrogen are combined.

The charge of "mongrel stock" is therefore groundless.

Guthrie's second criticism of our evidence is this, "It is not proved that the offspring may not have come from ovarian tissue of the host left in site after operation." But both the grafted animals were albinos and they were mated only with albino males. In all recorded cases, of which I have myself observed many hundred, albinos so mated produce only albino young. Had ovarian tissue been left in site after operation and liberated ova which developed, these should have produced *albino* young. But these grafted albinos, which had received an ovary from a colored animal, produced *colored* young, in each case of the particular color type that characterized the animal furnishing the graft. Is there really then any uncertainty about the source from which the functioning ova came?

W. E. CASTLE

LABORATORY OF GENETICS,  
BUSSEY INSTITUTION,  
HARVARD UNIVERSITY,  
June 21, 1911

#### MEASURING THE MERIT OF ENGLISH WRITING

TO THE EDITOR OF SCIENCE: Professor Thorndike's article in SCIENCE of June 18 on a scale for measuring the merit of English writing, seems to parallel the old question: "Which is best, a pair of scissors or a pair of tongs?"

To have any value as a test of merit the writing of "pupils in their teens" should be comparative, and you can not properly compare paragraphs based on different topics, recollections or quotations from school readers, and attempts at expression of totally distinct emotions.

One method which might approximate to a basis of comparison would be to require from all the pupils a paraphrase of one single paragraph, as far as possible to be expressed in entirely different words from the original. Even this would be subject to the objection that a child writes best when it writes of something it naturally appreciates, and in which its interest is not forced; and the same